

it, so far as it goes, is well selected and in the main correct.

Many a "creeper" and new hand on Eastern estates and elsewhere will owe Mr. Frank Braham a debt of gratitude for this handy little volume, which doubtless is the nucleus of a future work of more extended scope, not one with large type on art paper, but a planter's "Molesworth," of use to the old hand as well as to the beginner.

The author's section on general information will be found specially useful, and his essential rules for the preservation of health in tropical climates are admirable if intended for the young planter going out to the East for the first time; but for the older resident in the tropics "drink as little as possible—fluids inflate the bowel" is dangerous advice. The ingestion of a sufficient quantity of water or other bland liquid is essential if for no other reason than to counterbalance the effects of excessive perspiration. In hot weather the kidneys are very largely relieved of their excretory duties by the skin. If then the individual for years has been in the habit of still further relieving them by drinking as little as possible, it is not astonishing that in cases of emergency they are unable to return to their normal state of efficiency, and if blackwater fever is encountered death in such cases may be the result.

In these essential rules also mention of the all-important hot bath and change at sundown would have added to their completeness. The book, however, should command a ready sale.

Die moderne graphische Reproduktion. Ein Führer und Ratgeber durch das Gebiet des Illustrationswesens unter Berücksichtigung der für die Wiedergabe bestimmten Originale. Gemeinverständlich dargestellt von L. P. Mosler. Pp. vi+52+xiv plates. (Jena: Gustav Fischer, 1911.) Price 2 marks.

In this booklet the author gives a concise description of the methods of illustration as at present practised, so that those who desire to avail themselves of such methods may have some idea of their characteristics and possibilities, and be guided as to their choice. He refers also, shortly, to methods of making drawings and photographs for reproduction. Following the text are fourteen plates. Nine of these are impressions from half-tone blocks made from the same original to show the effect of the fineness of the screen, a reproduction of a reproduction, a "duplex" print, &c. There are also a four-colour print, a colotype, and a photogravure.

LETTERS TO THE EDITOR.

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The Distastefulness of *Anosia plexippus*.

REFERRING to the interesting note on "The Distastefulness of *Anosia plexippus*" in NATURE of October 12, I should like to suggest that the experiment, though interesting in itself, does not materially strengthen the case for the usefulness of mimicry. To make a good case for mimicry in the sense in which that term is ordinarily used, the mimic *Basilarchia archippus* should be tested and found palatable. Further, if mimicry means anything at all with reference to these two species, North American birds should eat some butterflies but not molest *Basilarchia archippus*. So far as observations have been reported, American birds eat butterflies very rarely, and there is no evidence, so far as I know, either from direct observa-

tion or from the thousands of stomach-content examinations made by the United States Department of Agriculture, that its non-mimetic relatives are eaten more often than *Basilarchia archippus*.

If European birds show the same indifference towards butterflies as is manifested by North American birds, either the "model," *Anosia plexippus*, or the mimic, *Basilarchia archippus*, might with impunity become widely disseminated in Europe.

A. M. BANTA.

Cold Spring Harbor, N.Y., November 20.

I HAVE a shrewd suspicion that if the result of my experiments upon *Anosia plexippus* had proved this butterfly to be highly palatable, the dwindling minority of mimicry sceptics would have made the very most of the fact, and claimed that it "materially strengthened the case" against Bates's theory of mimetic resemblances; and in this they would have been perfectly right, so far, at all events, as the particular instance is concerned. But since the experiments proved the precise opposite, those who believe in the theory may reasonably maintain that the case for the usefulness of the mimicry to *Basilarchia archippus* is thereby materially strengthened. It appears to me that few more important items of evidence in favour of the theory of mimicry can be established than demonstration of the distastefulness of "models."

Again, I think Mr. Banta overrates the negative evidence he cites in support of the view that birds seldom prey upon butterflies under natural conditions. My opinion to the contrary is based upon a large number of experiments made in the gardens of the Zoological Society of London, and published in the Proceedings of the society for September. I can recollect no exception to the rule that insectivorous birds were roused to attention at once by the sight of butterflies, their keenness suggesting very forcibly that they knew them, either by instinct or by experience, to be part of their natural diet.

Not less strongly suggestive was the determination with which they pursued, and the precision with which they generally caught, the butterflies on the wing when let loose in the shelterless aviary. Conversely, the skill evinced by the butterflies in dodging their pursuers in mid air was one of the most striking features of the experiments. Just as one is compelled to believe that the instinctive doubling of the coursed hare when overtaken by greyhounds indicates the habitual method of escape of the species from fleet-footed foes, so must one believe that the evasive twist of a butterfly when pursued by a bird is an inherited habit that has proved a means of salvation to members of that species in the past when chased by similar enemies. If, as is maintained, birds do not under natural conditions prey upon butterflies to a sufficient extent to be reckoned as serious enemies, what explanation is to be offered of the marked reaction observable between the two sets of animals when pitted against one another?

R. I. Pocock.

Zoological Society, December 6.

The Weather of 1911.

I HAVE studied with great care and interest Dr. Shaw's letter in NATURE of November 30 disclosing a promising field of inquiry into the causes of such abnormalities of weather as we have, for example, experienced during the summer and first half of the autumn of the present year. Seizing hold of the very fact Dr. Shaw's letter refers to, which has apparently induced him to look in the direction of the upper atmosphere above 9 kilometres for new lines of investigation, namely, that certain types of distribution of barometric pressure, normally rainy, occasioned little or no rainfall in the summer of 1911, I had before reading his letter entertained the idea that somehow or other the persistent, parching drought might have been due to an abnormally reduced absolute humidity of the atmosphere (at least of those strata not immediately in contact with surface water supplies) over these islands, so much reduced that the convection currents adequate to produce rain in more normal conditions failed to do so in the present year. I admit, of course, that a decreased absolute humidity of the air in hot summer weather, with the ocean all round us, would be a very different thing to account for, and perhaps could only be brought about by a persistent flow

of the air currents to our region from some arid continental region like the interior of Asia, which would be very unlikely in summer.

But the suggestions embodied in Dr. Shaw's communication no longer render necessary such an assumption as small vapour-content of the air to account for the circumstances of the drought. For I take it that if a barometric depression were transmitted to the surface strata entirely by variations of pressure occurring beyond the convection region at a height of about 9 kilometres, there would not be engendered the powerful rain-producing convection currents usually associated with cyclonic circulation when a barometric depression takes its origin wholly or partly in causes residing in the lower layers themselves.

Should this proposition not follow on the lines indicated in Dr. Shaw's letter, I should be glad if he would correct it.

L. C. W. BONACINA.

Hampstead, N.W., December 2.

MR. BONACINA would draw a distinction between barometric depressions transmitted from above and those which **take their origin wholly or partly in causes residing in the lower layers themselves**, and he would attribute a peculiarly rainy character to the latter. I know of no facts which enable us to draw with certainty the distinction in individual cases, and I should have been glad if Mr. Bonacina had cited some examples which were demonstrably of the second type. I cannot speak very positively on the subject. There is a certain hyperbolic region, sometimes shown on weather maps, between a pair of cyclones and alternating anticyclones where I think such depressions may occur. The weather associations of that particular distribution are peculiar; in the words of a well-known verse:

When it is good, it is very very good,
But when it is bad it is horrid.

In summer, thunderstorms with copious rainfall may occur with such a distribution. They are associated with areas of shallow depression—local secondaries—which may be entirely surface-bred; but the pressure differences involved are so slight that they must be regarded as belonging rather to the embroidery than to the outlines of the pressure distribution of the globe.

Let me take the opportunity to add that the further study of the physics of rainfall might contribute to the clearing up of some misconceptions about its meteorological significance. Suppose that we regard rainfall as incidental to convection, that is, to the process of righting instability in the lower or middle layers of the atmosphere, which may be caused by the superposition of layers of inverted density. Consider the effect of variations of pressure imposed entirely from above. If the lower layers contain a stratum of floating cloud, it follows from the difference of adiabatic temperature-change in cloudy and dry air that compression will produce instability at the lower surface of the cloud and rarefaction will produce instability at the upper surface. Alternate compression and rarefaction, such as would be imposed from above if some giant were playing upon the middle and lower layers of the atmosphere, with their cloud strata, like a concertina, would give alternating instability of different types. With one type rain would be produced. What would happen with the other I have not fully made out; perhaps the mammato-cumulus cloud may illustrate it. A good deal depends upon the vertical temperature gradient, of the causes of which at present we know little.

Certainly if there were no cloud layer to form a locus of instability, compression would simply raise the temperature of the air, and hence its potential dryness. The other stroke of the concertina, the rarefaction, might itself produce cloud, possibly at more than one level, and its continuance would maintain the cloud and the instability necessary for continuous, but fluctuating, rainfall if supplies of suitable air were forthcoming. We have here an explanation of cloud layers at different levels and of the well-known tendency towards cloud and rainfall with a falling barometer that is satisfactorily independent of surface conditions. Looking into the details, we can also see that a falling barometer alone is not necessarily a sufficient cause.

Parenthetically, may I express the hope that Mr. Bonacina or some other student of weather will refer to the figures for the dryness of the air of the past summer as compared with air under similar conditions of wind direction, &c., in previous summers? The facts are available and well worth investigation. There is, I have reason to think, a high negative correlation between humidity at 9 a.m. and the duration of sunshine for the day, and this may point to another factor in the cause of the brilliant summer.

I hope Mr. Dines will make good his view that the distribution of temperature in the upper layers is not related to the direction of the air currents. When I wrote I had in mind particularly a cold invasion in the upper air of July 29, 1908, with a northerly wind, and the apparent increase in intensity of the phenomena of a line squall at higher levels. But if we can be assured that cold invasions begin at the bottom, it will free us from much difficulty.

In the interesting letter from Dr. Carl Ramsauer published in your issue of December 14 the argument is based upon the assumption that during the past summer condensation in the atmosphere was inhibited for want of nuclei. The assumption requires proof before it can be accepted as a basis of physical reasoning. Supersaturation of air in the free atmosphere is often assumed, but, so far as I know, it has never been demonstrated, and meteorologists, as a rule, do not feel themselves at liberty to use it.

W. N. SHAW.

December 19.

THE contributions in your columns to this subject are of great interest. I will venture, with your permission, to propound one or two questions suggested by the letter of Dr. Carl Ramsauer. On his hypothesis, would it not follow that all periods of minimum solar activity would be marked by weather of the 1911 type? Is this the case?

On the same assumption, it would seem to follow that the same type of weather would extend over the whole earth: has this been the case? We have, I think, heard of torrential rains in northern Italy in the beginning of summer, and of the early onset of winter in Canada.

Again, assuming this hypothesis, not the earth only, but all the planets would have been affected. Have the supposed snow-caps of Mars shown any shrinkage?

Failland, December 15.

EDW. FRY.

The Photography of H α during Solar Eclipses.

IN NATURE for December 7 (p. 182) the report of the council of the Royal Society is said to state that at the total solar eclipse of April 28 the observers sent out by the Joint Permanent Eclipse Expedition secured observations with a short-focus prismatic camera showing the whole of the hydrogen series, with the additional remark that "this is the first time H α has been photographed at an eclipse."

In the interest of accurate annotation, however, this statement of the council must be modified, as reference to the reports presented to the Royal Society of the eclipse of 1893 (West Africa) and 1898 (Vizianagur) will show that the H α line was recorded as a strong line on the photographs obtained with prismatic cameras at both these eclipses by the expeditions from the Solar Physics Observatory. It was also recorded by other observers at the Indian eclipse of 1898.

CHARLES P. BUTLER.

The Nematodes of the Thames.

REFERRING to my former communication on this subject, I am happy to say that I have at last discovered the host of at least one of the species of nematode found in the Thames. Mr. Chas. Todd, of Tottenham, has kindly sent me a sample of mud from near the Tower Bridge containing vast numbers of Tubificidae. Among these are *Limnodrilus hoffmeisteri*, *Tubifex campanulatus*, and other interesting fresh- or brackish-water worms. The nematode has so far been found only in Tubifex. It gains an entrance into the spermatheca, where it develops to the great inconvenience of its host.

HILDERIC FRIEND.

Swadlincote.